

River Continuum Concept Interviews, 24-27 February 2005

Vincent Resh (VR), Robin Vannote (RV), Wayne Minshall (WM), James Sedell (JS), Colbert Cushing (BC), Kenneth Cummins (KC)

Group Interview 1

VR: One of the things that became clear at that meeting was that the River Continuum Concept was not only the most cited paper, it was three times as cited as the second most cited paper. So, one of the things that was clear was that we wanted some kind of documentation about how this idea came about, and I think that this is what we are all here for. I thank you for all coming down here, and we're really anxious to hear your thoughts about this. One of the things we'd like to start off with, this is going to have multiple inputs, is basically how did you just get to know each other? The continuum was maybe twenty-five years ago, but this is something that happened long before that, your initial contacts with each other.

Robin Vannote: I think, most certainly, professional meetings are the principal interaction of initial contact. Also, graduate school ties, professor ties and interactions, along those lines. I think the River Continuum group that's here, it was the Boston meeting. I think Ken was the organizer of that one.

Ken Cummins: Yeah. Triple A S

RV: Michigan State University put those talks together.

Bert Cushing: Four of the people here gave papers at that first Boston one.

VR (1:39): Did you guys know each other before then? Ken, how did you know the people in the group, if you could just mention?

KC: Through major professors, probably.

VR: How about Wayne?

KC: I knew about the Doe Run Work that he had done, and I knew about Robin through Bob Ball, and Cush...now how did we get started?

BC: The first real recollection that I have is the Pymatuning

KC: Oh yeah! That's right. That's where it was.

BC: And the first time I met Wayne was at the Boston one. I, of course, bonded with him, but I first met him at the Boston one. I think that was the first place I met Robin too.

Wayne Minshall: Right. And you will recall that it wasn't me that did the Doe Run work. That was Minckley

KC: Oh ok! Yeah right. What's the other one?

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WM: Well, allocthonous detritus and so on. We were all recently out of graduate school, so at least I don't know if I'd met you before triple A S.

KC: I don't think so. It's all pretty fuzzy past history.

WM: And then Bert and I roomed together at the triple A S meeting.

VR (2:49): What year was the meeting?

All: 1969.

WM: So, some of my contacts were from a whole different angle than you folks. I know we started getting involved in the International Biological Program, and for some reason, we were doing an exchange with students, and we showed up at the hotel in Richland. You were there. You came over with some groups. I was talking about some of the work we were doing, and for some reason, we started getting back together again. Where the fuzziness for me is that Rockefeller meeting. Because you guys had done that, and done some meetings and so on, and maybe melded together.

RV: You were on sabbatical. You were on a post-doc.

BC: It had to be between Boston in '69 and the OSU '72 meeting. The Stroud was somewhere in that...

Jim Sedell: I thought it was after the OSU meeting.

KC: What was the IBP meeting? Was that the Oregon State one?

BC: Yeah.

RV: What year was that?

JS: That was '73. That was the spring of '73, because that was the first time I ever met Franklin. Because I thought the Rockefeller came after that one. And it was after that one that we decided we could pull something together. We wrote the little grant to the NSF to meet at Gull Lake...

KC: That's right. We were supported for that meeting at Gull Lake.

JS: Right. But I met Ken and got into streams because of Ken when I came to graduate school at Pittsburgh. And then I stayed when you went to Michigan State because I didn't want to go through all that orals and comps stuff again. Then, as I was getting ready to leave, I got a job with Jim Hall and Norm Anderson there at Oregon State on the IBP project, and so I needed to take a tour, and I went up to see Stuart Fisher, and they say "You've gotta see this Stroud." I'd heard lots of stuff about Stroud, but never been there. And so then I went and spent three or four days at your place. That's when I got

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taken totally in. You I'd known, again, through papers, and I'd heard about you through the isotope stuff, and then through Dan Nelson.

BC: I don't remember where I first met him, but I can visualize it. He came down a row of seats in an auditorium, and introduced himself, but I can't remember where it was. (Everyone laughs) I remember that.

KC: It was at the meeting at Stroud that Charles Warren came, right?

JS: No. Charles Warren never left State, but when I got to Oregon State, it became real apparent that what they'd done for streams in the IBP was pretty parochial. It was fish, insects, and I wasn't going to make a living as a post-doc doing that, or being part of this project. So, Charles Warren was the big stream kahuna at Oregon State. He'd just published this book on water pollution in streams. And he had this, quote, theory. And I couldn't figure it out. I couldn't understand what he was trying to say, and so we suggested that he present what he was doing, and then we would bring a group from Michigan State, and group from Stroud, and the other place. And they all came, and we listened to Charles Warren, and weren't sure we understood him any better...

JS: So we had thought that in many ways it was a lot of mental masturbation, that there wasn't much there, and that the side conversations were far more interesting and important when we broke for coffee or had beers. That led to some thinking of ideas. I don't about...

BC: The Stroud was after the Oregon meeting?

KC: Right.

JS: We were out at the arboretum, and I think we went out to Berry Creek to look at that site, and it had kind of fallen in disrepair at that time.

BC: And they had that little chamber that you could sit down and look out and watch the stream go by.

VR (7:25): Robin, were you at this meeting?

RV: I was at the Berry Creek, and I remember Warren's talk and...Did he have the model at that time?

JS: Yeah. He had a family of curves. And if you took out a shotgun, and fired it at a wall he'd say "Ah! A family of curves! These are parallel curves!" But how did you decide where the point fit? Maybe it should have been on something else. I mean you couldn't ever...but you could draw a line through it.

RV: I think the major event that led my group at Stroud into the stream group here was probably the very fortunate event that we didn't get funding for IBP in our proposal. We

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got an atomic absorption for five thousand dollars as a consolation prize, and we took the proposal to the Rockefeller foundation, and they were very very excited about it, and at the conclusion of our study, they required that we bring in about fifty scientists in this area and show the results of the study, and I think that's what brought in a really exciting group of people that I've interacted with for many years afterwards. You were, unfortunately, I think, at post over in England, was that '72? I remember Bob Wetzel was there, the Oakridge group was there. It was a very fortunate meeting for me...

JS: I don't think I was there...If indeed it was before the Warren meeting, then I certainly wasn't there. And then you had another meeting afterwards, where you brought Tom Dunn and other people in.

VR (9:33): Bert, were you at that meeting?

BC: No.

WM: But that's what I think really tied Robin into the whole operation, really, the context that you guys had and then the liaison you did between the two.

RV: I think that pointed out to me the importance of bringing groups of people in to look at projects, and tell storylines, get feedback, and hear what they're doing, on that scale of a group, of forty...twenty to forty people interacting in a narrower field than a national meeting.

VR (10:19): How did you organize it? Did you have, like, papers giving the overview and then just have general discussions or groups?

RV: Well, we essentially had our story of the White Clay Creek, and we presented maybe half a day of what we had found out in our studying. It was a capsule presentation. And then, some of the other groups gave little talks. It was very informal, though. The room wasn't even as large as this (laughs). There were forty people, I think! But, it was a lot of fun, and most certainly from that meeting, ideas flowed about how can we interact more often. Then, I think we did get together at other meetings.

KC: Then, we started meeting at the different sites once we got going. I remember that Idaho State was the first time we played Frisbee football I think...

BC: But, you're saying that was still before we started as the "continuum" thing.

KC: No. This was me actually planning, because I remember Brock was at that meeting. We were planning...some kind of planning meeting about...or maybe, you know, we had a lot of proposal writing meetings where we would get together and write proposals.

WM: So, the key ones were Gull Lake and Stroud.

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BC: Okay, now we're starting what, in my chronology, is the first time THIS group actually got together and interacted.

KC: And we tried to write...the proposal that was rejected in...

BC: In '73...I guess I'm luckier because I had to get the work on this chronology for that talk I gave on it. So that was in '73, and I remember getting a call from Jim inviting me to it. We're going all to Gull Lake and we're going to try to get a group proposal. The NSF was receptive to something like this. But as I also recall, we did an awful lot of wheel spinning at that one, because what do we do at your site that compares to this, and mine in the desert, and so forth.

WM: And, of course, we were still sorting each other out. And there was no paradigm, nothing to really hang things on before that.

BC: And, as I recall, and as my notes say, all we really tried to decide to do was have another meeting at Stroud, and each of us would go back there and cover an area. Wayne and Robin were going to do invertebrates. Ken was going to do detritus. You were going to do geomorph, and I was going to do primary production. And we were all...we were still pretty, as I think, quite tunnel-visioned. We were still looking at these eight areas, same areas of expertise. What could we do to these flowing streams in different areas?

RV: One of the driving forces was the IBP money was drying up, and how were we going to get funding? Could we somehow join forces to come up with an idea of how to look at cross-biome comparisons. The difficulty was to come up with what parameters we were going to select to measure, and there was a lot of differences in concerns, because everybody had their favorite things to do.

BC: And even though we were supposed to come back and talk about the state of the art, those regions, it was still just that. It still wasn't really tied together.

WM: Well, the IBP brought us the framework of watershed level studies and nutrient cycling and energy flow, sort of, focuses. So I guess some of our orientation coming to the table was with that background and that experience. It was largely the driver of the IBP and the connections that I made through IBP that brought me into the...

JS: When I got back and found what I was as a post-doc at Oregon State was what I call fairly conventional fish or insect things. We turned that into much more an interaction with forest, using fishers of Bear Creek energy budget...carbon budget. And so that then allowed us to nest within a coniferous forest biome legitimately, and it just didn't look like disciplines were just attached to the coniferous forest biome. It became a real integral part of how that went about, but that also allowed us to start thinking about something else other than just communities of insects.

KC: You had some key players at Oregon State. You had Jerry Franklin, but you also had Fred Swanson and some other people who brought...all these places the geomorph input was pretty significant. I know the first time I met Fred, he said "Now let me get this straight, you really care about things that happen on an hourly or daily scale?" And I said "Yeah, yeah, we do." He said "If it's shorter than a hundred years, I can't really think about that." But I remember going out with you and Hank Froelick and measuring wood in streams. And that was a completely new kind of thing. So a lot was going, and I know that Franklin was involved heavily in IBP as was everybody there.

JS: Oh yeah. He and Dick Waring were Oregon State and the turf war was between Washington and Oregon State. The strategies were very different. Washington went with full professors and divied the funds out to full professors, and Oregon State purged the full professors and went with post-docs like Fred, myself and Frank Triska. Stan Gregory obviously emerged as a major find there.

WM: And at least in the history of the desert biome, the prima donnas were the dead ends. Basically, they wanted to do their own single focus kind of thing, and so going with post-docs was a really wise thing in IBP in the desert biome as with the younger professors, who also were more open to...

KC (16:57): A lot of people still say that IBP was a total waste of money, a failure. Franklin says "Well, no, because look at all the things that bloomed from that exercise. The meeting at Oregon State was the one that turned me around. That's the first time I had ever really gotten sort of the watershed perspective that you guys had, and the first time we met Franklin. Jerry's kind of a thread through this, including getting us funding when we'd been turned down by saying "No wait. This is a really good idea. We gotta get some money to these guys.

JS: Franklin never knew a good science story. I mean, he could spot a good science story. So his strength and still is to sit down with individuals, listen to what they've done and then weave a story from it. He could see that we were gathering the thread of a story here that he could weave with a terrestrial interface, whether it was different than what he'd been hearing through NSF meetings and things like that.

BC: But still, at that stage in our group, we were still looking. We were still spinning our wheels and having a hard time breaking out of what I thought was the conventional comparative – this, here and there. It wasn't until we went back to Stroud and invited a bunch of other folks to come back and share their thoughts with us that those papers (chuckles) as I remember, those papers really kind of fell by the wayside. I know I gave mine. I can remember giving it, but it seemed pretty dumb, quite frankly. It was: "Alright, here's the status of primary production now. So what?" I don't know if you guys ever even got up and gave yours, but by the time Robin really turned us on, that's where it really happened. I guess it was silly to give the rest of those, quite frankly.

KC: Yeah, it kind of lost its relevance.

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BC: Yeah. Maybe I did it out of pure bullheadedness, but I was gonna give mine.
(everyone laughs)

KC: I've prepared my slides, dammit, now I'm gonna show it!

BC: That's right. And that was a pretty eye-opening thing when Robin really turned us on to that whole paradigm.

WM: As I remember that, I'm not sure how much prelude Robin gave, but let's just say he was droning on, and it broke up in the back. Perhaps Jim and Ken and several others were talking, and meanwhile I was paying intense attention to it (*everyone laughs*), but anyway, Jim jumps up and says "Wait a minute! What did you say? Go back to that again." And so Robin repeats his little storyline, and that was, to me, the spark – the moment where they said "We got something that we can hang a lot of ideas and a lot of this background work we've been doing. We've got a framework now when we put it together."

VR (19:50): Robin, can you remember at all, in the short version, what the storyline you told was?

RV: Well, we had started out looking at a third order stream, and putting all our efforts there. Halfway through the study – this was a three year Rockefeller study – it became quickly evident that upstream processes and downstream transport were a major factor in what we were seeing on this riffle/pool. A typical study that you start off with, and it was just that in order to understand the system, you had to link the upstream communities, upstream habitats, and what was going on on the flood plains, or the riparian zone in the stream. We just found too many mice in the stream, so much leaf litter. The transport system was very important. You have all these filter feeders. It was easy to begin to link all of this together when you're living on a stream five to seven days a week – many many hours studying something intensely. It was just a fortunate place to work. And then I think you see what was happening, so I want to tell these guys what I thought of how a stream was working in its nine hundred hectare basin, which is a very small patch of landscape that had been only twenty percent forested, and eighty percent in farming. It was very fortunate to be there at that certain time.

BC: The thing I remembered, and I may be wrong here, but this is what hangs in my mind, is you presented largely a physical story, and the opportunity came up for us to hang biological analogs on this. My recollection was that way, that it was a very physically oriented story, and all of a sudden, the opportunity came that "Gee!" we could tie biology into this.

RV: I didn't really start interacting with Luna until maybe a few years later.

BC: Well, that's the perception that I keep.

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WM: Well, so help me with this. I thought part of the background, too, was that, whether it was a Rockefeller grant or whether it was another grant, you had an opportunity to go across the country looking at a variety of streams...maybe it was a sediment impact or a thermal impact...

RV: That was another very lucky...lucky...

WM: It was such a lucky thing. But...so the point was that in a very short period of time, a pressed period of time, he was able to look at a large number of streams.

RV: Yeah. The council on environmental equality wanted to evaluate the impact of five of the six programs from the soil conservation service, and the programs of the Army Corps on channelizing rivers. Forty-two streams were selected across the country, except in Mississippi. We weren't allowed to go near Mississippi (*everyone laughs*) but the chairman of the committee that deals with these sort of things was a Mississippian. But this gave me a very unique opportunity that select matched pairs of streams – a natural stream and a channelized stream – varying in size from a first order stream in South Dakota to the Kissimee to the King's River in California, and I saw how channelization affected stream communities, and how it affected sediment transport storage, what the meaning of a destruction of a ripple pool sequence, turning them into channels. That was a unique opportunity.

WM (24:41): Also, I'm surprised that you said that your interactions with Leopold were after this, because from the moment I met you, you were living and breathing geomorphology and thinking big scale sorts of things, and I remember explicitly when we did a little walkthrough at Michigan.

RV: There's a tape somewhere in USGS, I think it was in the late 1960's... '68 or '69 when the USGS came. The film team, much like this, and wanted a storyline of how Ruth Patrick and her group did stream surveys. They wanted it as a training program for USGS. I guess that's where I first met Luna, and then he would come out and teach a class at Penn for a couple of days, built that interaction.

BC: Bringing up Ruth's name, it also reminds me of a little sideline incident. We went out to have dinner that night, and Ruth was going to join us. As I recall – maybe correct me here – she wasn't wild about your idea for a while. You were a little reluctant to present some of these things, and then she sent word and said "I'm not going to be at the dinner" and if I remember, you relaxed quite a bit. (*everyone laughs*)

RV: Yeah I remember that. Ruth had a unique capacity to get money, and it was always good to be on the good side of Ruth.

KC: Came from money, and made lots of it.

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VR (26:40): I was wondering...you mentioned Jerry Franklin and Fred Swanson and briefly Stan Gregory. Who were some of the other people that when you were still in these planning stages...who else would you guys remember?

KC: In fact Stu ended up being a reviewer of the program right through the whole thing.

JS: It turns out that people working on parts of the proposal in different places.

RV: From Stroud lab, it was Tom Bott that had been doing studies.

KC: Certainly Bob Peterson at Michigan State.

WM: What about Larson?

BC: He was at Gull Lake for that first meeting. Rick and Tom. These were the only guys that sent a team to the first meeting.

VR: Rick who? Rick Larson?

KC: And Wetzel was at Kellogg at that time, so we had a lot of interaction with Wetzel.

WM: Pontification:

KC: Yeah. Pontification. But he was certainly tied up in geochemistry.

RV: Mike Kru...Klu...

WM: Mike Klugr.

RV: He did a lot. He changed themes a number of times, and he was at the Kellogg meeting.

KC: I really think that's how the microbiology got put in and the hypomyci fungi being the real keystone in the litter processing.

WM: So was Frank with you?

JS: Frank was with me, and he took part in the early discussions, but then once we got funded and a post-doc and a team, it became apparent that he was going to do his own thing and not mess with Bob...

KC: Was that IBP money that was sampling all those...detritus in all those streams?

JS: He was doing that. He and I went to grad school together, and he was in the Army, and then when he got out of the Army, I hired him at Oregon State to be partnered with me. The other person at Oregon State was Dave McIntire. *(group all begins talking)*

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WM: Jim Hall. In fact, Jim came to more of the meetings than either of the other two.

BC: Both of those guys were back at that Stroud, because I've got slides showing McIntire and Hall.

RV: I think Hall did a lot for us.

WM: Straight arrow as you kept referring to him. He kept you honest.

JS: Well, the way we worked at Oregon State and the way he did it was I would just roar out ahead, and he would...he had me on a choker collar. And so he would keep us honest, and ask for details. He asked tough questions – fill in the blanks...

BC: This was the question I was going to lead off the next...did you ever finish any of your ideas? (*everyone laughs*)

JS: My wife hired out people to do it for me.

RV: I think Norm Anderson also was near...he came to some meetings and I remember he came to Stroud and worked for a week or two on his type of work. I'm not sure why he didn't link up.

KC (29:57): Well, he was connected to the Oregon stuff all along at least peripherally.

JS: It's really interesting, the human dynamics, because when I came here, when I went to Oregon State, I was fortunate that as soon as I arrived, the people that were to supervise me and guide me intellectually all left on sabbatical. And the joke was that they came back, and they were working for me, because we totally transformed the study. We had these little bits and pieces of fish stuff, and insect, and nothing...and so we flipped it, and started doing a Stuart-Fisher kind of carbon budget that then we can attach the invertebrates to. We had a better story that fit with what we did. Norm was very very bent out of shape about it, essentially being thrown out. We stayed with him, and worked. I mean, he's still a very close friend now, and we all stayed together in meetings. So we worked hard at the group thing, even when really tough decisions were made. We talked a lot about that in the continuum – how we share credit, how we divvy up all kinds of things like that. So, people were concerned about how the dynamics of a group went, and how you try to keep a group together.

KC: What helped me was I took a sabbatical at Oregon State in '76. When I got there, I decided "I'm coming back here the first chance I get, because these guys..." That was my real first shot at a real multi-disciplinary thing. You guys had the whole kitchen sink...everybody. And nobody had the same ideas, and that was a time where...they were still getting paid to pull logs out of streams in Oregon in those days. That was a real culture shift that was science based. That's one of the most remarkable...and that's Forrest Service as well. It's a whole different philosophy of...something that was very

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bad was now actually the most important thing going on. All the players were there to make that science based thing happen. That was a real exciting place to be. I remember showing up, being the only person at Oregon State that didn't have a beard.

JS: Now you're all that's left!

KC: I still got one!

WM: I don't want to keep returning to IBP, but that was my part...

JS: That's how we connected with the desert biome.

WM: It was my interaction with them, I eventually became the aquatic coordinator for the desert biome, so I was working with a lot of aquatic people, trying to pull some sort of scheme together, at the same time interacting with the terrestrial people. I got this interactive land-water thing from that orientation. With the storyline we really developed on our research on Deep Creek and so on, we got together and you kinda got "Whoa! We got something there! Let's talk." Then I was invited to go to one of these meetings, more or less out of the blue. I kinda felt "Gee, you guys have all bonded during this Rockefeller meeting" and I hadn't been there, and this was really a lucky thing for me to have made that connection.

KC: IBP was real important. It gets a bad rap a lot, and I don't understand that, because it may have facilitated a lot of this.

WM: The bad part, I think, came from this vision of modeling everything, and our belief that we just do it and hand it over to the modelers and they'll take care of it, and they couldn't do that. They lost track, and I think we got a lot of negative stuff from that. But if you look at the fundamental research and the interactions that were built and the broader scale of working, both at watershed level and beyond, but also in the team efforts, that's really where I cut my teeth.

JS: At the time, there was a world-wide split also, in that the international IBP was still into energy budget or trophics, and the Europeans were very concerned that they hadn't made the links, where even though the modelers were going rampant in the US, they would turn to nutrient cycling, carbon cycling, nitrogen...they were using different techniques to get at it, and so they were moving real fast.

WM: Any time we get a technology breakthrough, I think...you know now, with all this genomics stuff, the tool drives the science for a long time instead of the other way around. And I think that's what some of our work did, then we'd say "Flip out the modelers, they can work for us," maybe, but it's not working that way now, with the top-down direction with the modelers telling you how to perform.

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JS: Ninety percent, at one time, of the NSF proposals in limnology had something to do with C14, and primary production. So that was in the early seventies, and then we started to flip around to detrital...

KC: We went to C13 so you could still do it.

WM: Yeah that's a great tool. What a powerful tool to have stable isotopes.

JS: But it was ninety percent of the money on ten percent of the problem.

KC: We were talking about it on the way over, the new genetic influx into ecology and fisheries and in DNA signature. It's another great case of "Here's a great tool, show me something to do with it." The problem is that the influx of the tool doesn't bring the philosophy and the interaction with it. Since molecular biologists are double recessive for eliminating ecology, you don't want to infect a system with that without some kind of way to make sure that ecology's still there. It's a great tool, but unless you've got the kinds of things we've been talking about...to me IBP brought a lot of people together. Some of the papers were good, and some of the papers were bad, and so on.

JS: But the interesting thing, there were interdisciplinary pockets that didn't get funded by IBP. You were part of the desert biome, right? That here, things were really going. Here, Stuart Fisher really never was, and I thought he broke some ground there at Bear Creek, there. It was just a matter of finding one another, you know?

RV: Well, it seems to me that many of the IBP projects could document and quantitate how a square meter was structured and functioned, and model that square meter, but there wasn't linkage with any other upslope, down slope areas. I think that science took a big step in looking at systems after IBP.

WM: It's true that the River Continuum idea that you presented is what brought about the linkages. What we were still talking about – the reach of a stream, or a riffle or whatever, and looking at that dynamic, and we did not make the connection, and we did not scale up to other sizes of streams.

RV: But the thing that was going on on the prairies, the Smokey Mountains, you knew what the...

JS: Respiration of a leaf was. (*everyone laughs*)

RV: Right.

WM: I think that's what you brought to the table, again, the fact that you'd looked at these streams across the nation, and you saw the dynamics operating basically similar in each of those, and so you could see a paradigm or a model there that tied those together. Many of the others of us were still looking at our own little world. The desert biome versus coniferous forest, or whatever.

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BC: That led, to me, to what was the most exciting, I think, week I ever spent with you guys. That was when we went back to Stroud, or no, it was Gull Lake, and took over George Lauf's house to write the proposal. That was really something. Remember hiring those women...house...what were they? Those typists. Those poor gals. Everybody was in a room, writing a piece of this and "What are we going to measure?"

WM: What year was that?

BC: '74. July '74.

KC: And Lauf's mother was like your mother, very sharp, so she was wandering around this group, and George kept trying to explain to her what was happening. She said "What are all these people doing in my house?"

BC: That, to me, I remember, was something. There was more generation, and everybody working like this, and typing girls who didn't know the language...

VR (39:16): Before we go into that, can we go back to that meeting where Jim said "Robin, repeat that" and lead from that into the meeting you're talking about.

BC: That's kind of where we are. Robin presented this idea, we got all excited about it, we said "We gotta go back home and think in each of our little areas that we were most interested in, of how it fit into this into a downstream-upstream linkage sort of thing." And then we met back there to write the proposal and tried to put all of this together, because we were still looking at "What are we going to measure for the primary production?" because that led to the workshop on primary production, where we were still grappling about "Are we going to do upstream-downstream oxygen chambers?" all this stuff.

JS: We went right to the details. (*everyone laughs*)

RV: Everyone had their favorite way of doing it.

BC: So that's what led. From that meeting, we all went home and started thinking linkage and upstream-downstream.

WM: Did we just hear from Robin on the last hour of the last day and then we just went home?

BC: It wasn't much more than that, as I remember.

RV: We spent a whole day on it. (*everyone talks*)

JS: The details bore me, but when you tell a story about it, yeah! I can see a story! I could talk to a taxi cab driver, and start to make some sense about what we were trying to

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do after we saw that, and so that was important, because I really do believe that at our best, we tell stories, and so he told a story, and it was a compelling story, and then it was a matter of how you shape it. But it was the only story we'd heard. We'd been together a couple of times, and all of a sudden, he launched it.

RV: I was saving it for another year... (*everyone laughs*)

JS: So we kept pushing to squeeze it out of him.

RV: Luna would be the best person to talk to about how it came out.

WM: Why do you say that?

RV: Um...I don't know (*everyone laughs*)

BC: Anyway, it led to that next Gull Lake thing, where the proposal was actually written, what we were going to measure, where we were going to measure it, but I remember for another reason that these other guys probably won't, but that's when we came up with "We're going to do a continuum from low order to high order in the biomes," and that's where I didn't fit in. Because the Columbia wasn't going to be workable, and the little desert streams weren't going to be workable, because they didn't have a continuum.

VR (42:20): So this is the Stroud meeting you're talking about?

BC: Yeah.

WM: Alright, Bert, come off of it. We'll take care of you. (*everyone laughs*)

BC: You guys could have dumped me there, probably, but instead I worked with Wayne and was given some overall responsibilities for some things.

WM: That's really where that triple A S meeting was important, because that's where we met, and we really hit it off for some reason. I dunno, Montana – Idaho kind of connection, or whatever. And then through the IBP, we further bonded, and then we've been friends all the rest of our lives.

VR: You remember Louis Tralta making the statement "You need somebody to hold one end of the Seine."

JS: Well, we can get into that later, but just group karma, group ability to hang together needs a whole different kinds of buffers at different times, and other individuals when we feel boxed in a corner, you could talk to, it can kind of be intermediaries. Bert, aside from the technical expertise, provided a lot of that buffering and interpretation and things that sort of was important.

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WM: Stability and carry-through, just a whole lot of things that were really important.

KC: George Lauf should be thanked for turning his house over, though. He said “You guys have got to get away. You’ve got to get out of this lab. You’ve got to have some place to go. I’ll just give you my house. And I’ll just go stay with someone else...but my mom still has to be there. Take care of her every now and then.” That was great.

WM: And I don’t have a good memory of that.

KC: She was up on a ladder, don’t you remember?

WM: And that was ’74...I remember the castle, being up in the castle. That was different from Lauf’s house, right?

KC: No. The one with the skylight and the shower?

WM: Yeah.

KC: Yeah, that’s his home.

WM: Oh. Okay. Alright. It’s a beautiful place.

KC: I remember his eighty-nine year old mother, up on a stepladder trying to change a bulb while we were having this meeting. It was a good setting.

BC: That was a wild session. Everyone trying to put it together.

KC: Felt like the Washington Post. Going to press. We were always on a deadline for that thing.

RV: We didn’t have some of the pieces put together, because Jim and I stayed up until five in the morning at some point.

JS: We redid one, got it in. We went back to Stroud, because you guys were all there, and we hired another secretary, and I became the go-between, and that was organized. We delivered it, and then we waited. Finally we got this call from Franklin, that it was on the bubble, but really rejected. Franklin liked the idea, he really liked the storyline, could we come in and talk to him? We talked to him, and I think that created another Stroud deadline, because we could always grab a train at six in the morning to dump it off, hand deliver it.

WM: Somebody hand carried it down there.

JS: Oh yeah.

KC: Franklin pulled it out of the ashes, and said “This is an important idea.”

WM: It was director’s prerogative. So he overrode, basically, the panel.

KC: Now, was the second cycle when we had to go to the board because it was over a million dollars? I remember the big thing about renewal being...the renewal was over a million.

JS: There was a track record. But those were the days when the NSF had a little more discretion. I mean, Callahan was there and he was an early skeptic, but then became a huge advocate, and then Mel Dyer followed Jerry Franklin, and the Amazon work that Jeff Richey and that group came on, and that again was one in which people like Bill Lewis just hammered it, because it was a big picture story, and it only had a couple of preliminary data points for the largest river in the world. They were drawing strange curves, and he had a meltdown and Dyer said “We need to take a chance on some of this to get moving in big science.” So NSF had directors at that time that were willing to take a little more risk than what they’re allowed to legally now, or whatnot.

WM: Probably, the directors now would take the risk if they could. It’s just that they can’t anymore. Mt. St. Helens might have been one of those hand-slapping incidents where some risks were taken, and it just bombed. What a waste. We’re not ever going to do that again without a full proposal and all this stuff laid out, and so on. I’m not sure that everyone would agree with that, but...

JS: Well that’s another one where they did, and they didn’t, because there were a number of NSF proposals that stemmed out of St. Helens that did persist for several years, but the group never coalesced into a big proposal. It was either colonization of insects or plants, or what happened in the ash zone, or there was little lake surveys.

KC: Who signed the paper that got the grass seed thrown out?

JS: Well that was NRCS. That was a political decision to show everyone that we were doing something for the world.

KC: That’s what fed the aquatic insects. And the elk. Grass seed. In a system, it would never support anything.

VR (48:11): So, these two meetings...you went to Gull Lake. Before you went to Gull Lake, you sort of worked out your methods that you brought to Gull Lake. You spent five days working on the proposal at Gull Lake, and then how long a break before you went back and met at Stroud to finish the proposal up?

KC: It was pretty quick, wasn’t it?

JS: Yeah. It was only a few days that we did it. Then we all went back, got rest, rethought it, and then we came back to Stroud to finish it off.

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WM: Must have been the dead of winter, because we have photographs of people in heavy coats and such. And then we left Kellogg in cold. But now, this is the second Gull Lake meeting, right? When we talked about detail and methods and so on?

KC: When we were at Laufs was in July.

WM: Yeah. So I just wanted to separate for Vince that there are two different Gull Lake contacts here. It's the second one we're talking about.

VR: Which one did you stay at Lauf's house?

Everyone: The second one.

BC: That's when Ken and Wayne wore their Omar the Tentmaker pants. Awful looking pants.

WM: Bellbottoms and plaid. Plaid! That's right...

KC: Yeah. We were class, no question.

BC: I shouldn't talk, because there's a picture at the PR workshop, and I've got a pair of plaid pants on.

VR: Well, this is sort of a remarkable story about Franklin overriding it, too.

JS: You need to talk to Jerry on his story on that. There was no question, because, again, the frustration they had was that there were no storylines coming out. It was more same-old same-old, only with better technology, or to get more precision on the measurement, but not... a concept change, and how we were looking at the world differently. And that's what Jerry saw in the continuum. We were moving a different way of looking at the world. That was worth taking a chance to fund, and he was also committed to this cross-biome comparisons. He's always big on that.

WM: So, he played two key roles. One was the workshop money. However that was laundered, or brought to bear...

JS: Well, we wrote a proposal, and submitted it from Oregon State.

WM: But you must have had some encouragement to do so, right?

JS: Yeah, from you guys.

WM: You think so? I thought you were on the inside track with Jerry.

JS: Well, we vetted it with him, and he said he'd entertain a proposal to get together to

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see if we had anything. And that was key. So, we wrote up a little proposal, said what we were going to do...

KC: At that was what funded the Gull Lake stuff.

WM: And the Stroud meeting.

RV: We all went down to NSF for a conference meeting with them. Jerry called about the reviews...

JS: We did two. One to go down and talk to them, privately, so that he could get a feeling of whether it was worth our while to continue, and then the second one was when we got the reviews back. He then told us what he needed if he was going to try to push it forward anyway.

VR (51:34): There wasn't actually a re-submittal?

JS: No.

VR: And Jerry Franklin was the program manager of ecosystems?

JS: Yep.

BC: I think it might be also for the record here, to point out that at that Gull Lake thing, not only did we write the proposal, but an integral part of that was determining the responsibilities of the PIs at the four biomes and that we were going to hire a post-doc to coordinate this whole thing.

VR: Just the five of you were at that meeting at Gull Lake?

WM: No. Klug was there. Bott was there. I remember interacting quite a bit with Tom in writing parts of it, and so I think maybe he was there.

KC: Peterson was in Sweden.

WM: Yeah. On one of these times, though, that's where I met him. You said "I've got this guy I want you to meet," and so one of those two meetings...

BC: It must have been the second one.

RV: Well, Bob was at Pittsburgh too, wasn't he? Because he started his job some time after you did.

JS: Right, but he followed Ken to Michigan State. He'd been just there a year.

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WM: So he was out there, and you had gotten a big detritus study going. So he was out there helping you with the lab streams as well as Dusty Creek.